

of the work, is the generous concession made to English and American scientific readers, by the kindness of the Swedish Government, who have caused this Memoir to be printed throughout in a double column, the left hand of every page being printed in Swedish and the right hand in English.

Much praise is also due to the editor and author for the careful manner in which the English portion of the work has been passed through the press, and for the very great care bestowed on its translation from the original Swedish.

CHARLES DARWIN

Charles Darwin. By Grant Allen. "English Worthies," Edited by Andrew Lang. (London: Longmans, Green, and Co., 1885.)

Charles Darwin, und sein Verhältniss zu Deutschland. Von Dr. Ernst Kraus. Darwinistische Schriften, Nr. 16. (Leipzig: Ernst Günther, 1885.)

IT is a curious illustration of the change which has passed over the English mind, that already the name of Darwin should head the list of a projected series of popular books, not on eminent men of science, but on "English Worthies." This first member of the series is, as might have been expected from its authorship, a pleasing and favourable specimen of a kind of literature for which the public appear to entertain so keen a relish. For it is not only clear and picturesque in style, but is also evidently written *con amore*. Indeed, it was impossible for any man of common sense or common sensibility to have come into any kind of relation with Mr. Darwin, without being stirred by feelings of hero-worship, and Mr. Allen's reverential love for the hero is a natural tribute fittingly rendered to the lofty nature and mighty influence for whose loss the universal grief is still so fresh.

As a biographical sketch the little volume is decidedly a success. It gives in brief compass and good language the history of Mr. Darwin's antecedents, of his life and work, of his relation to contemporary thinkers, and of his presumable influence upon subsequent thought. All of which is done without losing sight of the desirability, in a popular treatise, of upholding the element of romance—a kind of treatment to which the character, the life, and the work of Darwin unite in lending themselves, as it were, by nature.

In his review of the course of thought upon the theory of evolution prior to Darwin, Mr. Allen is judicious; and his speculations upon the probable position of this theory at the present time if Darwin had not lived, are interesting—tending, as they do, to show how indispensable was the work of the great naturalist in focusing the facts and showing the method. Or, to quote a somewhat happy metaphor of his own, "Darwin was not, as most people falsely imagine, the Moses of evolutionism, the prime mover in the biological revolution; he was the Joshua who led the world of thinkers and workers into full fruition of that promised land which earlier investigators had but dimly descried from the Pisgah-top of conjectural speculation."

Almost the only criticisms we have to advance relate to matters of opinion. Thus, for instance, the following passage seems to us absurd:—"Strange to say, the

abortive theory [of Pangenesis] appeared some years later than Herbert Spencer's magnificent all-sided conception of 'Physiological Units,' put forth to meet the self-same difficulty. But while Darwin's hypothesis is rudely materialistic, Herbert Spencer's is built up by an acute and subtle analytical perception of all the analogous facts in universal nature. It is a singular instance of a crude and essentially unphilosophic conception endeavouring to replace a finished and delicate philosophical idea." Now we can very well understand any one who has read both the theories including them in the same condemnation, as too highly speculative, devoid of verification, and so forth. But we cannot understand any one thus exalting the one to the disparagement of the other—and least of all so on the ground that Darwin's version is "rudely materialistic." Where can there be room for any other element than the "materialistic" in the case of an hypothesis which has to do with facts purely physiological? The objection to Spencer's version we have always taken to consist precisely in its "acute and subtle perception of all the analogous facts in universal nature," whereby we are gradually translated beyond the world of physiology altogether, until we may exclaim with St. Paul—"Whether I am in the body or out of the body I cannot tell."

And this leads us to a second criticism of a more general nature. Mr. Allen, we think, is too fond of comparing the work of Darwin and Spencer, and when doing so appears to us to attach an altogether undue merit to what he calls the "deductive" as distinguished from the "inductive" method. The work of these two great Englishmen is so unlike that, even though it has been expended upon the same subject-matter, it always seems to us a great mistake to compare them; we might almost as well seek to compare the work of an historian with that of a poet. "What an ex-tra-or-dinary wealth of thought that man has," was once observed to the present writer by Mr. Darwin: "when I first read his 'Principles of Biology' I was speechless with admiration; but on reading it again I felt in almost every chapter—Why, there is here at least ten years' work for verification." Now this is surely a sound judgment, and one, moreover, in no way disparaging to the genius of Mr. Spencer. But if it is a sound judgment, surely also it shows the mistake of comparing his genius with that of the man who wrote the passage more than once quoted by Mr. Allen—"After five years' work I allowed myself to speculate on the subject."

Again, with reference to the relative values in biology of the deductive and inductive methods, Mr. Allen appears to us behind the age. To quote only one passage, he says:—"The English intelligence in particular shows itself as a rule congenitally incapable of appreciating the superior logical certitude of the deductive method. Englishmen will not even believe that the square on the hypotenuse is equal to the squares on the containing sides until they have measured and weighed, as well as they are able by rude experimental devices, a few selected pieces of rudely shaped rectangular paper." Now, it is easy to sustain the doctrine here implicated with examples drawn from Euclid; but biology is not mathematics, and if any one truth more than another is necessarily and forcibly brought home to the intelligence

of a biologist—be he “English” or otherwise—it is the truth that in his science it is safer to cut out his materials in the way of experiment, than it is to build up his propositions in the way of deduction. Therefore, it is not without good reason that a proved “soundness” in this way of inductive research should be regarded as the best title to a place among men of science as distinguished from men of letters. “To be sound,” says our author, “is everywhere of incalculable value;” and to be sound in the present sense, “to have approved one’s self to the slow and cautious intelligence of the Philistine classes, is a mighty spear and shield for a strong man; but in England, and above all in scientific England, it is absolutely indispensable to the thinker who would accomplish any great revolution. Soundness is to the world of science what respectability is to the world of business—the *sine quâ non* for successfully gaining even a hearing from established personages.” And long may it continue so. Surely this acknowledgment of the supremacy of the inductive over the deductive methods has been gained by a sufficiently long struggle in the past, and surely the tardiness of this acknowledgment has been fraught with evils sufficiently conspicuous to render somewhat grotesque the term “Philistine classes” as thus applied to the devotees of observation and experiment.

There is only one other passage upon which we have anything resembling a criticism to pass, and we notice it the more readily because, while it relates to a somewhat important matter of fact, the fact is one the unwitting and quite excusable misstatement of which by the present biographer furnishes a good opportunity for rendering its true complexion. In his chapter on “The Period of Incubation of the Origin of Species” Mr. Allen says:—

“His way was to make all sure behind him, to summon up all his facts in irresistible array, and never to set out upon a public progress until he was secure against all possible attacks of the ever-watchful and alert enemy in the rear. Few men would have had strength of mind enough to resist the temptation offered by the publication of the ‘*Vestiges of Creation*,’ and the extraordinary success attained by so flabby a presentation of the evolutionary case: Darwin resisted it, and he did wisely. We may, however, take it for granted, I doubt not, that it was the appearance and success of Chambers’ invertebrate book which induced Darwin, in 1844 (the year of its publication), to enlarge his short notes ‘into a sketch of the conclusions which then seemed to him probable.’ This sketch he showed to Dr. (now Sir Joseph) Hooker, no doubt as a precaution to insure his own claim of priority against any future possible competitor. And having thus eased his mind for the moment, he continued to observe, to read, to devour *Transactions*, to collate instances, with indefatigable persistence for fifteen years longer.”

Now, we have quoted the whole of this passage because it serves to convey, in clearly expressed language, what is a very general misapprehension with regard to the length of “the incubation period.” But Mr. Darwin has himself told the present writer that the reason why he was so long in publishing his theory was simply because he wished to be fully persuaded in his own mind as to its truth before he incurred the moral responsibility of giving it to the world. Most of all those twenty years were occupied in collecting evidence, and in that process of self-criticism which he used to call “meditation,” with the single-minded view of self-persuasion. Here was surely a nobler motive, and

one more worthy of an “inductive mind,” than that of accumulating evidence merely in order to make out a good “case.” We doubt whether the popularity of the “*Vestiges*” exercised the smallest influence upon Mr. Darwin’s motives. He had no desire to make a stir merely in order to secure a literary success; and therefore he felt that the more attention his work was likely to attract the more pernicious was it likely to prove, unless it was throughout founded upon truth. Neither was he actuated by any petty regard for priority. The reason why he showed his notes to Dr. Hooker was because he entertained a higher regard both for the learning and the judgment of this friend than he did for those of any other man.

By a curious coincidence Dr. Kraus’s biography of Darwin appears in Germany about the same day as Mr. Allen’s in England. As we have thus received the two by consecutive posts, it is impossible to avoid comparing them. And the comparison is interesting, as showing the differences between the public tastes to which the biographies are respectively addressed. While the English volume is a pleasing sketch of a great life, the German counterpart is an honest piece of history. Dr. Kraus has spared no pains in making his work thorough. He has carried his investigations through the smallest detail of Mr. Darwin’s life and labours; he has brought together a number of letters written by Lyell, Hooker, Haeckel, Müller, &c., and also by Darwin himself; he has given a methodical account of the opinions entertained upon Darwinism by all the naturalists of any note in Europe and America who have either written or spoken upon the subject; and he has done all this without losing sight of the strong personal interest which attaches to the character of the immortal Englishman.

Many of Mr. Darwin’s own letters just alluded to are translations of those written to Prof. Henslow during the voyage of H.M.S. *Beagle*, and printed for private circulation among the Fellows of the Philosophical Society of Cambridge. But all the others are translations of letters now printed for the first time—the originals having been lent for this purpose to Dr. Kraus by Haeckel, Preyer, Fritz Müller, and others. These letters are all more or less effective in displaying the distinctive qualities of their author’s mind; but if we were requested to indicate one more than another which is of interest in this respect, we should mention the one to Haeckel in which the following passage occurs. The original English is not given:—

“Ich hoffe, dass Sie mich nicht für unverschämt halten werden, wenn ich eine kritische Bemerkung mache: Einige Ihrer Bemerkungen über verschiedene Autoren erscheinen mir zu streng, obwohl ich kein gutes Urteil über diesen Gegenstand habe, da ich ein so kümmerlicher Schulknabe im Deutschlesen bin. Ich habe indessen von verschiedenen ausgezeichneten Autoritäten und Bewunderern Ihres Werkes Klagen über die Härte Ihrer Kritiken vernommen. Dies scheint mir recht unglücklich, denn ich habe seit lange beobachtet, dass grosse Strenge die Leser verführt, die Partei der angegriffenen Person zu ergreifen. Ich kann mich bestimmter Fälle erinnern, in denen Herbigkeit direkt das Gegenteil der beabsichtigten Wirkung hervorbrachte. Mit Sicherheit empfinde ich, dass unser guter Freund Huxley, obgleich er viel Einfluss besitzt, noch weit grösseren haben würde, wenn er gemässigter gewesen und weniger häufig zu Angriffsen

übergegangen wäre. Da Sie sicherlich eine grosse Rolle in der Wissenschaft spielen werden, so erlauben Sie mir, als älterem Mann, Sie ernstlich zu bitten, über das nachzudenken, was ich zu sagen gewagt habe. Ich weiss, dass es leicht ist zu predigen und scheue mich nicht, zu sagen, dass, wenn ich das Vermögen besässe, mit treffender Schärfe zu schreiben, ich meinen Triumph darin setzen würde, den armen Teufeln das Innere nach aussen zu kehren und ihre ganze Albernheit blosszustellen. Nichtsdestoweniger bin ich überzeugt, dass dies Vermögen nicht gut thut, sondern einzig Schmerz verursacht. Ich möchte hinzufügen, dass es mir, da wir täglich Männer von denselben Voraussetzungen zu entgegengesetzten Schlüssen kommen sehen, als eine zweifelhafte Vorsicht erscheint, zu positiv über irgend einen komplizierten Gegenstand zu sprechen, wie sehr sich auch ein Mensch von der Wahrheit seiner eigenen Schlüsse überzeugt fühlen mag. Und nun, können Sie mir meine Freimütigkeit vergeben? Obgleich wir einander nur ein einziges mal begegnet sind, schreibe ich Ihnen, wie einem alten Freunde, denn das sind meine Empfindungen Ihnen gegenüber."

The chief value of the German biography consists in its setting forth the early recognition, the rapid spread, and the present acceptance of Darwinism in Germany. Dr. Kraus has always an easy case where he is displaying the old truth about a prophet among his own kindred. It was not until after we had well stoned our prophet that the nation began to recognise the reality of his mission; and, as Dr. Kraus remarks, it was not until after we had lost him that England was awakened to the true magnitude of her greatest son. So it was that, Samson-like, he slaughtered his enemies even in his death, and this on a scale which would have astonished no one more than himself, could he have lived to see it.

Dr. Kraus's narrative everywhere glows with an enthusiastic admiration of Mr. Darwin's character, and on this account he deems no trait of thought, expression, or even of movement, too trivial for the purpose of rendering a mind's-eye portrait to his reader. On the whole, this word-painting is accurate, and the workmanship in good taste. As he himself remarks, however, exception may perhaps be taken in the latter respect to his having entered upon the religious opinions of the naturalist. But as he has only collected material upon this subject which had already been published, and as he re-publishes this material in an excellent spirit of toleration towards all varieties of religious belief, we do not ourselves think that he can be justly said to have overstepped the limits of good feeling.

From this brief notice it may be gathered that Dr. Kraus's book is both a thorough and an interesting piece of biographical work; and we must not forget to add that its interest is enhanced by two portraits of Darwin (one, the last that was taken, and the other a likeness of him as a young man), a picture of his house in Kent, and a facsimile of one of his letters.

GEORGE J. ROMANES

OUR BOOK SHELF

British Zoophytes; an Introduction to the Hydroida, Actinozoa, and Polyzoa found in Great Britain, Ireland, and the Channel Islands. By A. S. Pennington, F.L.S. (London: L. Reeve and Co., 1885.)

THE object of this book is to furnish a handy, and at the same time reliable, manual of British zoophytes, using

this term in somewhat the same sense as Dr. Landsborough did; and the author aims at making it do for the present generation of students what the reverend doctor's "Popular History of British Zoophytes" did for those of a former one.

In so far as the object of the author has been to furnish a catalogue of the Polyzoa and most of the Coelenterata of the British Isles, this has been fairly fulfilled, and, as far as we have been able to judge, the catalogue is in most instances a reliable one; but the student will not find it a ready help to the determination of the species; for though in most cases the diagnoses of the genera are given, yet it is but rarely that there is enough of a hint given as to the specific characteristics of a form to enable its name to be even guessed at; so that the working biologist interested in naming the species he collects must still have by him the works of Gosse, Hincks, and Busk. The usefulness of this volume would undoubtedly have been vastly increased if the labour had been gone through of giving analytical tables of both the genera and species, and it seems to us very undesirable that new species should be introduced into a work like this without detailed diagnosis. The size of the volume need not have been greatly increased if a uniform diagnosis of the species had been attempted, for then no doubt would have been curtailed the quotations, often of no scientific value, from the writings of Dalyell and others.

We have also to regret that the list of the habitats seems to us not to have been judiciously selected. Thus, in the case of some of the rarer forms, it is not unusual to find the exact English localities given, but these followed by such indefinite indications as "Irish" or "Scotch" coasts.

In the introductory chapter we find a somewhat ambitious attempt to write the history of the progress made from 1599 to the present time in our knowledge of "zoophytes." We have no wish to be critical on the facts mentioned, but to find the writings of Trembley, Peyssonnel, Réaumur, Ellis, and Fabricius quoted, and the name of Esper, emphatically the eighteenth-century authority on this "group," not even alluded to, strikes us as curious.

As long as the author had the writings of Hincks, Busk, or Gosse to depend on, there he has been at his ease; but in the few cases where he has had to go unaided, as among the Alcyonaria, it is evident that he would have been the better for some help. In such instances, as indeed all through his work, he would have found more assistance from "Carus Prodomus Faunæ Mediterraneæ" than from isolated papers in our scientific journals.

The bibliography in Appendix A is quite unworthy of the name. From it alone no student would, without assistance, find out even what the authors wrote about. Fancy bibliographical references in these modern days, and in a work written for the present generation, of this style:—

1742. Réaumur, "Histoire des Insectes."

1821. Deslongchamps, "Encyclopédie Méthodique."

1838. Milne-Edwards, "Recherches sur les Polyps."

1864. Rev. A. M. Norman, "Contributions to 'Ann. of Nat. Hist.,' &c."

1884. Andrés, "Die Actinien."

In Appendix B—the glossary—many words are given without any explanation of their meaning; thus, while we learn that *aperture* is "an opening or orifice," and that *orifice* is an "opening," that *apex* is "the top of anything," &c., we have such words as the following left unexplained: *avicularia*, *bathymetrical*, *calyx*, *epistome*, *funiculus*, and so on.

It is just on such matters as we criticise that we have a right to expect in a compilation that care should be taken. The general usefulness of such a volume depends on the way in which each detail is worked out. Motives that the reader of the preface will understand make us